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*Time to Tenure: Does
Tenure Reform Affect
Teacher Absence
Behavior and
Mobility?*

D a n G o l d h a b e r
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Time To Tenure: Does Tenure Reform Affect Teacher Absence Behavior and Mobility?

Dan Goldhaber

University of Washington Bothell/American Institutes for Research

Michael Hansen

The Brookings Institution

Joe Walch

University of Washington

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Dan Goldhaber, Michael Hansen, Joe Walch

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Abstract

We rely on natural experiments in North Carolina and Washington State, which previously extended time to tenure by one year, to estimate models that assess the relationship between the extended probationary period and absence and attrition outcomes for teachers affected by the new tenure laws. Across both states we find evidence of decreases in teacher absences for probationary teachers who are subject to the new extended tenure laws, and in Washington, we find a significant reduction in absences in the specific year in which tenure was extended. We find mixed evidence for teacher attrition and mobility.

1. Introduction

Tenure is a common job protection for public school teachers in the United States. Nearly all states provide teachers who achieve state specified years of seniority are afforded with specific opportunities (due process) to defend themselves against what they may see as unjust dismissals.¹ The job security afforded by tenure may also be a feature of teaching that helps attract high-quality individuals into the profession. But tenure, and the associated costs of due process dismissals, is seen by some as a key impediment to achieving a high-quality teacher workforce, both because it reduces the incentives for teachers to work hard and makes it more difficult to fire poor performers (e.g. McGuin, 2010; Weisberg et al., 2009). Indeed a central argument by the plaintiffs in the high-profile *Vergara* trial (*Vergara v. California*, 2014) was that the tenure provisions in the California code result in the state employing too many “grossly ineffective” teachers who also tend to be clustered in schools serving disadvantaged students. The state, by contrast, argued that because tenure provides necessary protections for teachers, tenure laws under attack “have been good for public education and for kids” (Frosch, 2016).²

Reform of teacher tenure has become a significant focus of education reform. Kansas, for instance, legislatively eliminated tenure in 2014, a change that is being appealed to the state supreme court (Felton, 2016a). In New Jersey, Governor Christie has recently asked the state supreme court to

¹ Tenure is by no means unique to public school teachers. It is also common in higher education and government service. Arguments for tenure in academia rest on preserving academic freedom, signaling quality to the labor market, and mitigating asymmetric information in hiring new professors (Carmichael, 1988; McPherson and Schapiro, 1999; Waldman, 1990); in public service occupations, tenure is an important safeguard against public officials using public jobs as an avenue of patronage to supporters (Chubb and Moe, 1988).

² *Vergara* struck down tenure-related provisions of the California Education Code as unconstitutional in 2014. However, this decision was overturned in the California Court of Appeals in February 2016. The plaintiffs brought the case to the California Supreme Court to reconsider the reversal, but the Supreme Court refused to try the case. For a more comprehensive description of *Vergara* and other changes to states’ tenure laws, see Goldhaber and Walch (2016).

eliminate tenure in districts with low student achievement (Felton, 2016b). In fact, more than half of the states with tenure guarantees have weakened tenure-based job protections since 2010, representing the most active rollback of tenure-related job protection dating back to 1909, the year in which New Jersey enacted the first comprehensive statewide tenure law.³

Changes to tenure laws vary: some states have eliminated tenure entirely or tied tenure to student achievement, but the most commonplace type of reform is lengthening the time until a teacher receives tenure. Most states with tenure provide teachers with due process job protections after they have completed three years of teaching within a state, but over the last five years at least eight states have pushed this timeline back by at least a year. The theoretical arguments for this reform are that (1) teachers will exert greater effort knowing that policymakers will have additional time to evaluate their performance prior to making a tenure determination, and (2) policymakers will have more information to judge teacher performance prior to making tenure decisions.⁴ Yet, as we elaborate in the next section, there is little empirical evidence on the consequences of tenure reform.

In this paper we describe research that investigates the effects of extending the time before which teachers receive tenure (the probationary period) on teacher effort, proxied by absence behavior. We also assess the likelihood that different types of teachers progress through the probationary period to become tenured. We rely on natural experiments in North Carolina and Washington State, each of which previously extended time to tenure by one year, to estimate difference-in-difference models that show whether there is an association between the extension of time to tenure and outcomes for teachers who are in cohorts affected by the new laws.

³ One impetus for this was the U.S. Department of Education's Race to the Top program (Kahlenberg, 2015), which includes language specifying that evaluations be used to inform decisions regarding "removing ineffective tenured and untenured teachers and principals after they have had ample opportunities to improve, and ensuring that such decisions are made using rigorous standards and streamlined, transparent, and fair procedures" (Section D2.iv.d).

⁴ A central argument by the plaintiffs in the Vergara trial was that policymakers need *more time* to assess teachers' performance and potential.

Across both states we find evidence of decreases in teacher absences for teachers who are in their early years of “in-district experience” (experience that counts toward tenure) who are subject to the new extended tenure laws.⁵ In Washington, we find a significant reduction in absences *in the specific year in which tenure was extended*, but this is not the case in North Carolina. In order to test whether these findings might be related to unobserved factors concurrent with the passage of the laws extending tenure, we check whether a placebo treatment that assumes the law was implemented a year early in each state. The findings hold up under this falsification test.

We also assess whether there appears to be differential patterns of mobility or retention of teachers associated with the extension of time to tenure. Specifically, we first estimate multinomial logit models to compare pre-tenure mobility patterns among teachers before and after the change in tenure policy. We find evidence that pre-tenure teachers were significantly more likely to move between districts after the law change in North Carolina, but no evidence of different mobility patterns in Washington. We also estimate logit models testing whether there are changes in the likelihood of the attrition from districts for teachers with different absence behaviors, on the assumption that absences may be a criteria used in selecting teachers for tenure that may be affected by the extension of the probationary period. In North Carolina, we find a significant relationship between the likelihood of attrition and absences – teachers with more absences are significantly more likely to attrit – but little evidence that this is related to the time to tenure. The patterns are less clear in Washington, but here too there is little consistent evidence of a change in attrition that is related to the tenure extension.

2. Theory and Evidence on Teacher Tenure

⁵ We use the terms “in-district experience” and “experience towards tenure” interchangeably.

A large body of research shows that differences in teacher quality can have profound impacts on student achievement. Teacher quality is the most important resource over which schools have control; the effects of having a particularly effective or ineffective teacher swamp those of class size changes (Goldhaber, 2002; Rivkin et al., 2005) and by some estimates can be equivalent to more than a full grade-level of student test achievement (Hanushek, 1992). And value-added measures of teacher effectiveness have been shown to predict a variety of important later life outcomes (Chetty, et al., 2014).⁶

Arguments about the potential impacts of tenure reform are pretty straightforward, though as we describe below there is limited empirical evidence that directly speaks to their merits, particularly extensions of time to tenure. On one side of the debate are the teachers unions (American Federation of Teachers, 2003; National Education Association, 2015), as well as most teachers (Farkas et al., 2003), which consider the due process provisions of tenure an important deterrent to unjust dismissals.⁷ In theory, the job security afforded by tenure should make teaching a more attractive profession than it would be otherwise, perhaps drawing more talent into teaching (Rothstein, 2015). On the other side, tenure reform advocates (Paul, 2009) make two distinct arguments against rewarding tenure early in a teacher's career: first they argue tenure mutes the economic incentives for individuals to be productive on the job, and second, they argue that tenure limits a key workforce quality management tool – teacher dismissal – by making it prohibitively costly to fire a teacher. Consequently, it is common to see calls for extending the pre-tenure probationary period (or eliminating tenure altogether) so that policymakers have a longer window of time to assess a teacher's potential (Miller and Chait, 2008) and

⁶ Outcomes such as teen pregnancy, college-going behavior, and labor market earnings.

⁷ The practice of rewarding tenure is not unique to public school teachers, but is also common practice in both higher education and in government service. The theoretical reasoning for tenure practices in academia rest on preserving academic freedom, signaling quality to the labor market, and mitigating asymmetric information in hiring new professors (Carmichael, 1988; McPherson and Schapiro, 1999; Waldman, 1990); in public service occupations, tenure is an important safeguard against public officials using public jobs as an avenue of patronage to supporters (Chubb and Moe, 1988).

make better decisions about which teachers ought to be allowed to continue in the profession (Gordon et al., 2006; Hanushek, 2009, 2011).⁸

Arguments about the potential impact of tenure reform on the *effort level of teachers* is supported to some extent by a body of research from outside of education contexts showing that job protection policies like tenure affect worker effort, measured by their absence behavior (e.g. Ichino and Riphahn, 2005; Scoppa, 2010). There is also some evidence in teaching that absence behaviors respond to financial incentives or various changes in absence policies (Duflo and Hanna, 2005; Ehrenberg et al., 1991; Jacobson, 1989; Stoddard and Kuhn, 2008). Bradley et al. (2007) present evidence that teachers on temporary contracts (analogous to the probationary period we focus on) are absent about 20 percent less often than tenured teachers, all else equal.⁹ They also find evidence that teacher absences are positively correlated with their coworkers' absences, suggesting that some absence behavior (which they interpret as shirking) is dictated by workplace norms.

More pertinent to our analysis, Jacob (2010) finds that a change in the collective bargaining agreement in Chicago leading to reduced job security for more junior teachers (though no formal change in tenure timing) had a quantifiable impact on teacher absences, reducing them by about 10 percent.¹⁰ Jones (2015) comes closest to measuring the impact of time to tenure on effort levels of the existing teacher workforce. Using state-level variation in time to tenure to assess its relationship to the amount of time teachers spend with students and their parents outside of the classroom (a measure of teacher effort), this research concludes that in the year teachers are being evaluated for tenure, they

⁸ While most states grant teachers tenure after three years, there are a number that allow tenure after only one or two years (Miller and Chait, 2008; National Council on Teacher Quality 2008).

⁹ It is possible that the negative correlation on temporary contract status may be partially related to low experience or low unmeasured teacher productivity; the authors do not examine this.

¹⁰ Jacob also offers more tentative evidence that the policy change had a direct impact on student achievement that went beyond the effect that might be expected from the reduction in teacher absences, suggesting that teachers increased effort along a number of dimensions.

spend more of their own money and communicate more often with students and their parents. This additional effort, however, reverts to a baseline level after the receipt of tenure.

The ultimate concern about teacher absences is that they affect student achievement. The evidence on this is quite robust as teacher absences have been linked to student test scores in empirical research across several states and settings (Hoxby, 2002; Clotfelter et al., 2007; Miller et al., 2007; Stoddard and Kuhn, 2008). A particularly compelling study from Hermann and Rockoff (2010) estimates that the student achievement effects of teacher absences are comparable to replacing an average teacher with a 10th percentile math teacher or 20th percentile reading teacher for each absent day. And over the course of a school year, the accumulation of 10 absences is associated with a 2-3 percent reduction in student test scores (Clotfelter et al., 2007; Miller et al., 2007).

The second argument for extending the time to tenure is that it will give policymakers more time to evaluate teacher performance, allowing them to be more selective about which teachers are tenured and improve the composition of the permanent teacher workforce. The due process protections afforded by tenure make the dismissal of teachers for any cause costly and, as some argue, far less likely (Reeder, 2005; Matus, 2009).¹¹ While the typical legal costs of firing tenured teachers vary by state, it can often be several times a teacher's annual salary, exceeding \$250,000 (Associated Press, 2008). Extending the pre-tenure probationary period may improve school districts' ability to manage workforce quality through selectively retaining productive teachers and dismissing ineffective ones while the costs of dismissal are low.

There is little systematic evidence about whether probationary teachers lose their jobs or tenure protections related to their performance.¹² A number of recent papers, however, model the effects of

¹¹ There are significant barriers to dismissal in many districts (Griffith and McDougald, 2016), and very few tenured teachers are ever fired. For example, in Illinois, only 44 of over 100,000 tenured teachers were dismissed from 1991 to 1997 (Goldstein, 2001).

¹² This is perhaps why it is prominent news when school systems announce that teachers are being fired (e.g. Turque, 2011) or losing tenure protections (Felton, 2016c) as a result of poor performance.

selective retention policies, or “teacher deselection” (e.g. Goldhaber and Hansen, 2010; Hanushek, 2009; Staiger & Rockoff, 2010; Winters and Cowen, 2013). While the specific estimates vary, these simulations suggest that removing teachers with low value added has the potential to improve the quality of the teacher workforce enough to have an educationally meaningful impact on student test achievement.¹³ Chetty et al. (2014) estimate that replacing a teacher whose true value-added is in the bottom five percent with an average teacher would increase students' lifetime income by \$267,000 per classroom taught.

The above simulation studies consider the partial equilibrium effects of deselecting ineffective teachers, but not the broader ramifications of deselection policies, such as whether they impact the quality of teachers entering the profession through changes in the employment contract. Rothstein (2015) attempts to account for broader implications of reform, exploring, again through simulations, a variety of potential tenure policies. His study suggests that it is optimal for student achievement to have a three-year probationary period, but also that there is very little difference in the impact on student achievement of granting tenure after 2, 3, or 4 years of classroom experience.¹⁴

To our knowledge, there are only two papers that exploit changes in tenure policy over time to identify effects on the teacher workforce. Loeb et al. (2014) investigate the effect of tenure reforms in New York City, initiated in the 2009-10 school year, that made two major changes to NYC’s tenure process. First, it allowed administrators to selectively extend the probationary period for low-

¹³ The value of being more selective about who is retained in the teacher workforce depends on such factors as the signal to noise ratio of value added (hence the number of years of teacher value added that inform decisions) and the value added of those teachers that replace teachers that are deselected. Winters and Cowen (2013) find that dismissal policies have the potential to improve the quality of the teacher workforce by about .02 standard deviations of student achievement when replacing the bottom 5 percent of teachers with average teachers and about .05 standard deviations when replacing the bottom quartile with average teachers. Goldhaber and Hansen (2010) simulate a policy that replaces relatively novice teachers from the bottom quartile with average incoming teachers and conclude that such a policy could increase student achievement by about .03 standard deviations. Chetty et al. (2014) find that replacing a teacher in the bottom 5 percent of value added with an average teacher would increase the lifetime earnings of students by over \$250,000 per class.

¹⁴ The conclusion that it matters little whether tenure is granted after 2, 3, or 4 years is partially driven by what may be a questionable assumption: that the benefits of more information about teacher effectiveness are offset by some extent by ineffective teachers remaining in the teacher labor market longer when probationary periods are extended.

performing teachers, giving principals more time to gather information about performance; second, it increased district guidance in making recommendations for teacher tenure or tenure denial. The authors find a large decrease in the percentage of teachers approved for tenure after the reform, from 94 percent of eligible teachers in the two years prior to the reform to 56 percent of eligible teachers in the three years after the reform. They also find that teachers with extended probation were more likely to transfer to a different school or leave the profession altogether. Importantly, teachers who left the district were significantly less effective than their replacements in terms of both value added (by about 20 percent of a standard deviation of student achievement on ELA tests) and principal assessments.¹⁵

Strunk et al. (2016) focus on the effects of Louisiana's elimination (in 2012) of teacher tenure on teacher attrition. They find the removal of tenure increased the attrition of teachers affected by the reform by about 1.4 percentage points, or about 20 percent. Moreover, the estimated effect of the reform shows up only for teachers actually affected by the reform (e.g. not for junior charter school teachers who did not have tenure protections prior to the law change). Strunk et al. do not investigate whether the reform influences the type of teachers who attrit, but recent research strongly suggests that tenure reforms have the potential to influence student achievement through teacher effort and employment decisions. Dee and Wyckoff (2013), for instance, assess evaluation and teacher accountability reforms in Washington, DC (known as "Impact") and find that teachers under threat of dismissal are less likely to return to the district (Dee and Wyckoff, 2013). To our knowledge, however, ours is the first study to provide evidence on whether state-level policy changes that increase time to tenure affect teachers' absence behaviors.

¹⁵ Adnot et al. (2016) find a similar result on the effects of teacher turnover when investigating the Washington, DC IMPACT teacher performance assessment and incentive system. Teachers who exited the district after being sanctioned under IMPACT were replaced by more effective teachers, with an average effect of .08 standard deviations of math student achievement.

3. Data and Analytic Approach

To assess how time to tenure influences teacher effort and workforce effectiveness, we exploit policy changes in North Carolina and Washington State. **Table 1** outlines current requirements for tenure in each state and describes the details of each state's time to tenure reform. Under North Carolina's 1997 Excellent Schools Act (Section 11, North Carolina General Statutes 115C-325, ratified June 1997), the state extended the pre-tenure probationary period from three to four years for all public school teachers in the workforce who had not earned tenure by July 1998. Similarly, in 2010, lawmakers in Washington extended the pre-tenure probationary period for all teachers entering the workforce in the 2010-11 school year or later from two to three years (see SB 6696).

[Table 1 about here]

A. Data

The study relies on administrative databases from North Carolina and Washington that track teachers over time. The North Carolina data, maintained by the North Carolina Education Research Data Center at Duke University, contain information on all teachers in the state school system, spanning the 1994/95-2001/02 school years.¹⁶ The teacher data include information on teachers' educational backgrounds, demographics, licensure status and licensure test scores, teaching assignments, and

¹⁶ North Carolina teacher absence data is also available through 2007/08. We choose to limit the number of years in the analytic sample to make the panel comparable to the Washington panel and because including years long after the tenure law revision may conflate the effect of the law change with other factors, such as labor market conditions (e.g. Nagler et al., 2015)). Additionally, Clotfelter et al. (2007) find potential issues with the quality of the North Carolina absence data in some years after our panel.

teacher absences, our proxy measure for teacher effort. Teacher absences in North Carolina are recorded by type (e.g., administrative, sick, personal) and by pay period (one per month).¹⁷

The structure of the teacher data from Washington, which we obtained from Washington State's Office of the Superintendent of Public Instruction, is very similar to that from North Carolina. We observe teacher-level information (including education, licensure, demographics, and district) spanning the 2008/09 through 2014/15 school years. The data on teacher absences is from the Washington School Information Processing Cooperative (WSIPC) that includes teacher absence data (including the date of each absence), allowing us to use this measure as a proxy for teacher effort.¹⁸

The argument for using a teacher's total number of absences in a year as a measure of effort is straightforward: the measure has been shown to predict student achievement (Clotfelter et al., 2007; Miller et al., 2008), and teachers have at least some discretionary control over their own absence behavior. The timing of absences are potentially important. In Washington, collective bargaining agreements in many districts mandate that the district notify teachers by March 16th whether they will be offered a contract in the following school year. For teachers in their final probationary year, receiving such notice would be confirmation of tenure status for the following year. In theory, teachers face the most pressure to reduce absences prior to the March deadline during their last probationary year and less pressure after the notification date. For this reason, we choose to define absences during the extension year in Washington (teachers' third year in a district) by counting only absences before March 16th. The absences variable in other years includes the total number of absences throughout the entire school year.

¹⁷ Our primary absence models define total absences as days of sick leave plus personal leave, and we drop teachers with more than 150 absences per school year, consistent with Clotfelter et al. (2007).

¹⁸ The WSIPC dataset is not a complete census of Washington teachers, but it does include teacher absence information from more than half of the 295 school districts in Washington State. Not all WSIPC districts reported teacher absences in all years. The number of participating districts rose each year, with 59 districts reporting absences in 2008/09 and 163 districts reporting absences in 2014/15.

For the purposes of our study, we also compare the behavior of pre- and post-tenure teachers on two additional absence measures. First, we examine the number of absences after the administration of the state assessment in each year since post-tenure teachers face school test accountability pressures that diminish after students have sat for the state assessment, while pre-tenure teachers maintain their incentives to perform. Second, only in Washington, we measure the percentage of absences that fall on Mondays or Fridays. In particular, we focus on teacher absences that fall on Mondays or Fridays, since these are arguably more reflective of teachers' choices given that they fall around the weekend.¹⁹

We intentionally do not include student achievement on standardized tests as outcome variables in this analysis. Ideally, we would have preferred to evaluate the changes in tenure policies on these outcomes to present a more complete picture of the policy's effects; however, we have chosen to limit this analysis to teacher absences only for two reasons. The first, and most critical, reason for excluding student achievement is due to sample size issues. Only a minority of teachers in our K-12 sample are teaching in tested grades and subjects, a minimum requirement for estimating value-added models, and our efforts to estimate these effects showed findings of tenure extension coefficients with large standard errors (these issues were most apparent in fixed effects models). Second, and relatedly, while prior evidence shows that changes in teacher absence are associated with changes in student achievement, the estimated effect sizes suggest that we should only see quite small impacts on student achievement associated with the magnitudes of the changes in absence behavior that we find (e.g., Clotfelter et al., 2009). In other words, to the extent that the primary way in which the tenure extension influences student outcomes is through absences, we would not have the power to detect impacts on student achievement.

¹⁹ For instance, only about 40% of non-discretionary absences (e.g., serious illness) should fall on Mondays and Fridays if these absences are distributed randomly throughout the week.

Our analyses require that we identify teachers who have tenure and can identify which teachers are affected by the change in time to tenure. Unfortunately, it is not possible in either state to definitely determine which teachers are tenured; however, we can infer tenure status based on how long teachers have been employed in their district. **Table 2** shows the cohorts of teachers that are plausibly affected by changes in tenure laws and illustrates the tenure status for different cohorts at various points in time. This table represents school years down the table rows and years of in-district experience across columns. A teacher entering the workforce in the 1994/95 school year (in Panel A) and not moving districts is represented by moving diagonally down Panel A (from the upper left to lower right), incrementally gaining one year of in-district experience with each school year. The dark solid line represents the point of tenure for teachers as they gain teaching experience in a district. The shading in the table indicates the timing of tenure eligibility for each cohort, assuming teachers remained in the same district for their entire probationary period.²⁰ For both North Carolina and Washington, there is one cohort in which teachers were under the old law at the beginning of their careers, but entered their second year of teaching under the new law, making them subject to the extended probationary period.²¹ We also note that some teachers from earlier cohorts could also be subjected to a change in tenure policy during their probationary period: teachers who switch districts during the probationary period reset their tenure clock (returning to the first column of the table), and could therefore extend

²⁰ It is important to note that we define teachers as being under the “new law” in a particular year if they are required to teach for the extra probationary year before earning tenure, not that they were teaching in a year after the new law was passed. Teachers who had already earned tenure, or who would earn tenure if they remained in the same district before the cutoff period would not be subject to the new law, even if they were teaching in a school year after the law was implemented.

²¹ In North Carolina this was the 1996/97 cohort; the new law was read and ratified in June of 1997 and only applied to teachers who had not earned tenure by July 1998. The cohort entering in the 1997/98 school year presumably was applying and searching for jobs in the spring of 1997 in preparation to enter the teacher workforce under the old law, but by the time they actually started teaching in the fall they were subject to a new tenure regime. In Washington, the law was passed in March of 2010 and applied to teachers who had not earned tenure by the 2010/11 school year, which means that teachers in the 2009/10 cohort started their first year under the old law, but were under the new law when it was passed during their first year. It is possible that the tenure extension was more salient for teachers in these cohorts because of loss aversion. Teachers who had the expectation of a short probationary period when they entered the profession and “lost” the shorter probationary period when the new law was put in effect may respond differently to the new law than teachers in later cohorts who were aware of the longer probationary period.

their tenure period to four years, even though their cohorts were not originally affected by the tenure policy change.

[Table 2 about here]

In North Carolina, teachers must stay in one school system for the length of their probationary period. The change in the law exogenously changed the time to tenure for all teachers in one cohort of beginning teachers, but also extended time to tenure for teachers of the prior two cohorts who switched districts before tenure and were therefore not fully tenured prior to the policy change (see full detail in Joyce, 2000). Districts hiring *previously tenured* (but not probationary) teachers may either award tenure at the point of hire or wait one additional year. To account for this, we estimate models (described in Section 4C.) that include an indicator for ambiguous tenure status due to a district switch. As we describe in Section 4C our findings are not meaningfully affected by changing the assumption of districts' tenure decisions for teachers who transfer from other North Carolina districts. In Washington, there is no parallel ambiguity since all tenured teachers have a one-year probationary period if they switch districts.²²

In both states, we construct the analytic dataset by limiting the sample to only include teachers we observe entering the teacher workforce during the span of the longitudinal data available in either state, among eight cohorts entering the teacher workforce in either state around the time of the policy change.²³ In North Carolina, creating the sample this way means that we observe two cohorts of new

²² The tenure reform in Washington gives superintendents the discretion to grant tenure after two years if teachers receive one of the top two evaluation ratings in each year, starting in 2014-15. There were also a small number of districts that participated in a pilot program, which provided these districts with the option of using the new evaluation system before it was fully implemented. As a robustness check, we estimate all models excluding the 2014-15 school year and also robustness checks excluding these pilot districts, and find similar results to those from the full sample.

²³ In North Carolina, this means that we exclude teachers in 1994/95 with any unobserved experience, since it is unclear whether those years were spent teaching in the same district (and thus counted toward tenure) or if teachers

teachers who, if they do not move during their probationary period, will be tenured under the old law, and one cohort that entered under the old law, but was be subject to the new law by the end of their first year teaching. The remaining five successive cohorts entered teaching under the new law. In Washington, we observe three cohorts subject to the old law, one cohort that started their first year teaching under the old law but had their probationary period extended under the new law, and five cohorts that entered the workforce under the new law.

The primary analytic dataset we employ from North Carolina consists of 169,967 teacher-year observations (56,767 unique teachers), and the sample in Washington includes 43,317 teacher-year observations (14,358 unique teachers). **Table 3** provides select sample statistics for each state, broken out by tenure status under the old and new laws.²⁴ Consistent with existing research (Clotfelter et al., 2007; Hansen 2009; Jacob 2010), we find that tenured teachers have substantially more absences, by about 2 to 5 days, than non-tenured teachers. There is a small increase in the number of absences of probationary teachers in North Carolina after the new law, but one would not want to draw strong conclusions from this given that the probationary group of teachers now includes teachers with more experienced teachers who tend to have more absences. In Washington we see similar a similar difference between tenured and probationary teachers, but there is a larger difference in the number of absences between untenured teachers under the old law and untenured teachers under the new law. Part of this difference can be explained by the structure of the WA absence data. The WSIPC dataset only includes observations for teachers with recorded absences, so a teacher with no absences would not be present. We assume that a full-time classroom teacher who does not show up in the dataset has 0 absences, but it is possible that the teacher is missing because of a data entry error, in which case we

switched districts in 1994/95, resetting the tenure clock. However, it is possible include teachers with zero years of credited experience in 1994/95 since we know that is the first year in the district. The teacher personnel file in Washington goes back to 1985/86, so we exclude teachers with more than zero years of experience before 1985/86.

²⁴ The means in the table are based on teacher-year observations so teachers may appear in the data more than once.

would be incorrectly coding teachers with 0 absences. We take steps to check the accuracy of our assumption; for example, in our primary models we only include teachers from districts that started recording absences at the beginning of the school year. Even after our restrictions, there were many more teachers with 0 absences in early years of the dataset. We do a variety of robustness checks on our primary models (described in the following section): dropping teachers missing from the absence file, dropping teachers from districts with an unusually high number of teachers missing from the absence file, and dropping the first year in the sample. These models yield results that are qualitatively similar results to our primary models.

There are only small differences in the demographics of probationary and tenured teachers across tenure law regimes and little difference in the basic skill licensure test scores (the Praxis I in North Carolina and the WEST-B in Washington) between probationary and tenured teachers in either state under either tenure regime. This provides cursory evidence that the extension of time to tenure did not have a dramatic effect on observable characteristics of the cohorts of teachers entering the profession.

[Table 3 about here]

About 13 percent of probationary teachers in North Carolina and five percent of Washington teachers switch districts during their probationary period. This is important as the district switch resets a teacher's tenure clock in each case. More generally, the figures in Table 3 illustrate the importance of distinguishing between "overall experience," which is credited for salary determination regardless of whether teachers switch districts.²⁵ Overall average experience exceeds experience toward tenure by approximately 3 to 4 years in each state, and the correlation between these two measures of experience

²⁵ A teacher's placement on the salary schedule is determined by degree level and the number of years of overall experience (rather than experience within the district), which can be credited from teaching out-of-state or in private schools. For more details see the Revised Code of Washington 28A.400.300.

is only about 0.5 in North Carolina and 0.7 in Washington. One reason for this modest correlation is district switchers, but these two experience measures can also differ because teachers enter or leave the labor market, or are granted experience credit for teaching in other states or in private schools.

B. Analytic Approach

This study investigates the following research questions:

- 1) Is the change in time to tenure related to proxy measures of teacher effort (various measures of teacher absences) and how does teacher effort compare pre- and post-tenure and between states with different tenure criteria?
- 2) Is the extension of the probation period associated with differential teacher mobility patterns before tenure? Is the mobility of teachers with high absences during the pre-tenure period related to the policy changes?

Question 1 asks whether there is an observable response in quantifiable measures of teacher effort and time to tenure. We cannot directly observe teacher effort, but we argue that several measures of teacher absences are a good proxy for effort (or, more precisely, absences are a proxy for withholding effort or shirking). Specifically, we investigate: the number of absences in a school year, the number of absences after the administration of state tests, and the percentage of absences on Mondays or Fridays (in Washington).²⁶ These measures alone do not paint a complete picture of the relationship

²⁶ The absence date is only available in Washington State, so the days of the week measure can only be generated in that data. North Carolina reports absences by pay period (one pay period per month), for which absences in May and June will be used as the measure of post-test absences (by state statute, North Carolina standardized testing is carried out at least 20 school days before the end of the school year).

between tenure and effort, but absences certainly conform to the classical assumptions of effort in a moral hazard problem; that is, productive for the outcome of interest and manipulated at the discretion of the agent (Stoddard and Kuhn, 2008). Furthermore, as we noted above, absences appear to be at least partially discretionary and have been modeled as such previously.²⁷

The analytical models that will be used to address this first research question will build off of the following difference-in-difference regression:

$$a_{jt} = \beta_0 + X_{jt}\beta_1 + I_{\text{untenured}}\beta_2 + I_{\text{extended}}\beta_3 + I_{\text{untenured}} * I_{\text{extended}}\beta_4 + \varphi_t + \varepsilon_{jt} \quad (1)$$

In this equation, absences for teacher j in time t , as proxy measures of effort (a_{jt}) are predicted as a function of teacher characteristics (X_{jt}), being untenured ($I_{\text{untenured}}$), being exposed to the extended probationary period under the new law (I_{extended}), the difference-in-difference variable that interacts the two ($I_{\text{untenured}} * I_{\text{extended}}$), and a year fixed effect (φ_t) to account for overall changes in absence behavior across years. The vector of teacher characteristics includes indicators for teachers' gender, race/ethnicity, and holding a master's degree or higher; it also includes the number of years of experience that teachers are credited as having for salary purposes (note again that this can be different from experience toward tenure). The indicator variable on the extended probationary period takes on a value of 1 for any teacher where the new law would apply to the teacher if she remained in her current district until tenured. This variable does not reference the tenure regime under which a teacher is eventually tenured, and it does switch for teachers in affected cohorts.

This first model provides an overview of the relationships between tenured and untenured teachers by lumping all pre-tenure and post-tenure observations into binary groups. This can be

²⁷ This is not the first study to use teacher absences as a proxy measure of effort, which is also an explicit assumption in Hansen (2009), Jacob (2010), and Stoddard and Kuhn (2008). These studies all find evidence of teacher absences changing in ways that are consistent with theoretical predictions of effort.

problematic if changes in teacher effort are most pronounced just before or after the tenure point. To address the possibility of differential changes in effort relative to the tenure point, we also estimate models that estimate year-specific differences in district experience ($I_{\text{dist_exp}}$) in place of the untenured indicator variables, and include overall teacher experience as an indicator variable (I_{exp}) (both experience vectors are entered as sets of year-specific indicator variables up to five or more years of experience):

$$a_{jt} = \beta_0 + X_{jt}\beta_1 + I_{\text{dist_exp}}\beta_2 + I_{\text{dist_exp}} * I_{\text{extended}}\beta_3 + \varphi_t + \varepsilon_{jt} \quad (2)$$

Model (2) provides point estimates for teachers' absences at various points, both under the original law (using the vector $I_{\text{dist_exp}}$) and under the new law (under the interacted vector ($I_{\text{dist_exp}} * I_{\text{extended}}$)). The point estimates for the absence trajectories under the two different tenure regimes are identified through cross sectional variation across the pool of teachers in the sample. If extending the probationary period affected teachers' absences, we expect to see significant differences in the vector of difference-in-difference variables for years around the old and new tenure points. In this equation, estimates are identified off of variation observed across all teachers in the data, without regard to their entering cohort, and ignoring observations over time within the same teacher. We also estimate model (2) with school fixed effects to control for possible differences in absence behavior that may systematically vary across schools.

It is possible for the estimates on the parameters of interest to be affected by changes in the mix of individuals in the sample. Indeed, one possible consequence of the tenure reform is that different types of people chose to enter teaching, i.e. the tenure law affects workforce composition. For instance, lessening teachers' job security would be predicted, absent an offsetting compensating differential, to lead to lower ability teachers entering the workforce (Rothstein, 2015). On the other hand, a central

argument for extending time to tenure is that it gives school administrators more time to evaluate teacher performance, allowing them to make more informed tenure decisions – in other words, affecting the post-tenure workforce composition is an explicit goal of the policy.

We estimate two variants of (2) above to account for the potential selection in and out of the teacher workforce over time. First we include cohort fixed effects in the model. This specification, in which identification of the tenure extension variables is based on variation in exposure to tenure regimes across teachers entering in the same cohort, is designed to account for the potential that the extension of time to tenure affects those who enter the workforce. Identification of the tenure regime change variables within cohorts is driven by two sources, which varies depending on the specific cohort. In both states, some cohorts were untenured at the time of policy enactment but, if teachers did not switch districts during the untenured period, they could become tenured before the extended tenure period was implemented. In these cohorts, within-cohort variation in the tenure regime is driven exclusively by district movers in the probationary period who, by moving, reset their tenure clock under the new extended period. Additionally, there was one cohort in both states where teachers had already taught one year when the policy was enacted and they would not be eligible for tenure prior to implementation (i.e., their first year in the profession was under the old regime, but all future years were under the extended regime). For teachers in these cohorts, within-cohort variation comes from both the law going into effect (for all teachers) and district switchers who reset their tenure clock. Unaffected cohorts (those fully tenured before the law's enactment or entering the profession after it) will not contribute to the difference-in-difference estimates in the cohort fixed effects models. The drawback of this model, however, is that since the model is partially identified by movers (which could be non-random), our estimates may not reflect accurate counterfactuals across groups.

The second variant we estimate is a teacher fixed effects specification. Under this specification, point estimates are identified only by teachers personally affected by the extension of the probationary

period, which happens for probationary teachers in the workforce at the time of the policy's enactment in both states either by switching districts or due to the law change (again, this varies across teachers depending on the entry year into the workforce). As above, teachers already tenured before the law change or those entering the profession after the law change will not contribute to these estimates. These estimates will reflect within-teacher variation in absences, rather than changes in entry cohorts of teachers or those who leave the public school teacher workforces in North Carolina and Washington (by their own volition or through the decisions of administrators). Thus, this specification could be interpreted to reflect solely the incentive effect associated with the tenure change.²⁸ The drawback with this approach, though, is that we do not observe pre- and post-policy change counterfactuals within the same teachers. Rather, we observe teachers who began teaching under the shorter probationary period and switched to the longer probationary period, and our point estimates of the absence differences combine these estimates across teachers. Thus, these two specifications could tell us qualitatively different information about how teachers are responding to the changes, and neither model should necessarily be preferred.

Question 2 asks whether the extension of the probationary period in either state was associated with changes in pre-tenure teacher mobility in districts, either overall or specifically among teachers who we may expect may be at risk for being denied tenure. The first inquiry investigates changes in any teacher mobility during the probationary period associated with the enactment of the new tenure law. We estimate a multinomial logit model in which we predict the likelihood that pre-tenure teachers either switch districts (P_j^{switch}) or leave the state entirely (P_j^{leave}) as a function of their overall and in-district experience under the old and new tenure laws:

²⁸ As discussed, one source of identifying variation for the teacher fixed effects specification is teachers who begin their career under the old law and switch districts before earning tenure. A potential concern is that this group of teachers may be different than the group of teachers who remain in the same district until earning tenure. Even though the teacher fixed effects specification controls for any time-invariant observed or unobserved characteristics, this model does not account for potential differences between the two groups.

$$\begin{aligned}
a_{jt} &= \beta_0 + X_{jt}\beta_1 + I_{\text{dist_exp}}\beta_3 + I_{\text{dist_exp}} * I_{\text{extended}}\beta_4 + \varphi_t + \varepsilon_{jt} \\
P_j^h &= \frac{\exp(\alpha^h + X_{jt}\beta_1^h + I_{\text{dist_exp}}\beta_2^h + I_{\text{dist_exp}} * I_{\text{extended}}\beta_3^h + \varphi_t)}{\sum_{g \in \{Stay, Leave, Move\}} \{\exp(\alpha^g + X_{jt}\beta_1^g + I_{\text{dist_exp}}\beta_2^g + I_{\text{dist_exp}} * I_{\text{extended}}\beta_3^g + \varphi_t)\}} \\
&\text{for } h = Stay, Leave, Move \quad (3)
\end{aligned}$$

This model basically parallels regression equation (2), with the key difference being the use of mobility outcomes as the dependent variables in a multinomial logit.²⁹

In addition to general changes to mobility, we wish to investigate whether teachers may either select out or be selected out of the workforce differentially under the extended probationary period. Similar to the research questions related to teacher effort above, we will use teacher absences as a proxy measure of effort to model whether these measures during the pre-tenure period differentially predict teachers' retention in the district under the differing tenure time tables. Note that in neither state do we actually observe tenure decisions; rather, we only observe teachers in the district that have not yet qualified for tenure and those that have accumulated enough experience in the district for tenure to apply. Thus, we do not know the reasons why a teacher may choose to leave a district before receiving tenure—it may be a direct consequence of the tenure policy or be unrelated to it.

For this exercise, we estimate a teacher-level logit model where the dependent variable is whether a teacher left the district, conditioned on pre-tenure measures of absences:

$$PExit_{jt} = \beta_0 + X_{jt}\beta_1 + I_{\text{extended}}\beta_2 + Absences\beta_3 + Absences_{jt} * I_{\text{extended}}\beta_4 + \varepsilon_{jt} \quad (4)$$

²⁹ The analytic sample for Equation 2 in Washington includes school years 2009-2015. For Equation 3, the sample in Washington excludes the 2015 school year, since we do not have available data regarding teacher mobility after the 2015 school year.

This regression equation models the probability of a teacher j to exit the district in the following year ($PExit_{jt}$) for the earliest years of a teacher's experience in a given district. We combine the two different types of mobility into a single variable here, as both are equivalent to the district when deciding to whom tenure will be granted. Explanatory variables include teacher characteristics (X_{jt}), an indicator for the extended probationary period (I_{extended}), a vector of absence measures ($Absences_{jt}$), and the extended probationary period interacted with the absences vector. The absence vector contains two variables: the number of absences a teacher takes during the year (relative to the mean number of absences across all teachers in the year) and an indicator variable flagging teachers in the highest quintile of absences.

We estimate these models iteratively, among all teachers who are in their first year in the district counting towards tenure, all those in their second year, etc. up through one year following the extended tenure period in each state. We do this to examine whether any differences in selection arise between the two tenure regimes, and if so, what time periods they arise in. If districts were to use the additional probationary year as a tool to be more selective about rewarding tenure, we would expect most of the differences between the tenure regimes to arise in the extended year. However, another alternative is that the increased focus on performance before tenure could induce relatively weak teachers to leave the classroom years before the tenure decision. Finally, it is possible the extended period makes no difference in teacher mobility decisions at all, in which case none of the interacted variables would be statistically significant.

While the answers to all of these questions will provide estimates on the potential impact of reforming time to tenure on teacher workforce quality, we would appropriately caution readers against drawing strong causal inferences on all aspects of tenure reform. The primary reason for this caution is that teachers likely enter the profession with strong expectations about the employment contract and changes to tenure would entail major revisions to those expectations. Specifically, increasing the time to

tenure (or other reforms, such as using student test scores in determining tenure eligibility or the elimination of tenure altogether) would greatly impact the extent to which teachers (and prospective teachers) view teaching to be a profession with a high degree of job security. Consequently, we might expect these changes to have quite broad ramifications for the teaching profession; for instance, economic theory would suggest that fewer capable individuals would, all else equal, opt for the profession since it no longer offered such a high degree of job security. We do not plan to focus great attention on this issue, but will assess whether there is *prima facie* evidence of this by seeing whether there appears to be significant differences in observable characteristics of teachers hired before and after time to tenure changes.

A second reason for caution in interpreting these findings is that it is possible that other important education policy changes may have been enacted simultaneously with the extension of the probationary period in these states. In North Carolina, the change in the time to tenure law was enacted as part of a larger bill intended to improve educator quality, which included mandating changes for teacher preparation programs, certification rules, evaluation standards, and altering the statewide minimum salary schedule (e.g. see Joyce, 1997 for more detail on these changes). All of these simultaneous policy changes somewhat cloud our ability to claim a causal relationship in our analyses, yet we can directly account for some of these in the statistical models (e.g., by controlling for experience). Moreover, we stress that these changes were not implemented on the same schedule or on the same group of teachers as those affected by the change in probationary period.³⁰

³⁰ Of the many policy changes included in this bill, three appear to be particular threats to our research design, but we feel the specifics of implementation mitigate the confounding effect of these changes. First, the change in initial certification rules specifically affects novice teachers, but by law teachers eligible for tenure (our treatment group) will already be beyond the initial certificate stage and have a continuing certification. Second, the law mandates the State Board of Education to implement uniform evaluation procedures (which could vary by tenure status), but the schedule for implementation is left to the State Board of Education and local school boards. In North Carolina, as is true for most school districts (The New Teacher Project, 2009), there is little evidence that school districts actually implemented evaluations that led to differentiated performance ratings during the late 1990s. And third, the changes to the state minimum salary schedule may influence the composition of the workforce; yet, state salaries are observed in the data and can be included as control variables. See full details in Joyce (1997).

In Washington State, the tenure law was changed as a part of legislation (SB 6696) designed to make the state's Race to the Top application more competitive. The main focus of this legislation was on teacher evaluation; specifically, it required school districts to adopt a new four-tiered teacher evaluation system. Importantly for our purposes, however, the change in time to tenure takes effect for untenured teachers in the 2010-11 school year while the new evaluation system is only required in 2013-14. This may afford us the ability to distinguish between time to tenure effects and the effects of changes to the evaluation system, though it is possible that districts began focusing on reforming their evaluation systems prior to the time that the state required they be in place. We are unaware of changes to any other state policies that would dramatically affect the quality of the teacher workforce in these two states (and each of the authors has considerable history of doing policy research in these states), but given that we cannot definitely rule out the potential that there may be unobserved *concurrent* policy changes affecting teachers or students, we would of course appropriately caveat our findings.³¹

4. Results

A. Tenure Extension and Teacher Absences

We focus on how the change in the law affects teachers whose behavior may be directly incited by the changes in the time to tenure – focusing on *experience toward tenure* – and below (in Section 4B) on whether there are different mobility patterns for teachers whose behavior might have been affected by the reform. We begin our discussion of the results by looking first at the estimated

³¹ Unobserved, concurrent factors are always a problem with difference-in-differences research designs (Bertrand, et al., 2004). Note, however, that such confounding factors are only a problem to the extent that they *differentially affect* the 'treatment' and 'control' groups. For instance, one may worry that an exceptionally stormy winter in the post-tenure year may increase absences among newly tenured teachers for exogenous reasons, but as long as the stormy weather affects both the newly tenured teachers and previously tenured teachers equally, this does not pose a serious threat to our identification strategy.

relationships between time to tenure and teacher absence behaviors. The first set of results is presented in **Table 4**, where the dependent variable is the total number of absences during the school year.³² Columns 1-4 represent various regression specifications using the North Carolina absence data and Columns 5-8 use data from Washington.³³

[Table 4 about here]

The results in Columns 1 and 5 include vectors of both in-district experience towards tenure and a vector of in-district experience (experience toward tenure) interacted with the new tenure law.³⁴ In these specifications the excluded reference category is teachers with zero years of experience toward tenure *and* zero years of any type of experience credited to them. In both states we find that teacher absences tend to significantly increase with both in-district experience towards tenure and overall experience. For instance, the estimated difference between a teacher with 4 years of prior experience in the same district and a beginning teacher is calculated by summing the coefficients on the 4 years in-district experience and 4 years overall experience variables; in North Carolina, this estimated difference

³² The North Carolina absence dataset includes separate categories for sick leave and personal leave that are consistent statewide and summed to construct the dependent variable used in these primary models (consistent with Clotfelter et al., 2007). However, the absence categories in Washington are designated at the district-level and are not completely consistent across districts. We make inferences based on absence descriptions to construct the absence dependent variable in Washington to be similar to the definition used for North Carolina. As discussed in Section 3A, in Washington the absence variable for the transition year (2 prior years of experience) only includes absences before March 16th.

³³ As described in Equations 1 and 2, these regression models include year fixed effects, as absences did show significant variation across years. We also estimated the models without year fixed effects and saw very different results; we prefer these models with year fixed effects.

³⁴ To clarify the interpretation of the experience variable definitions: teachers in their first year in a district have 0 years of experience toward tenure, teachers in their second year have 1 year of experience, teachers in their third year have 2 years of experience, etc. The tenure reform in North Carolina extended the probationary from 3 years to 4 years (so the 3 years of experience category represents the law transition year), and the reform in Washington extended the probationary from 2 years to 3 years (so the 2 years of experience category represents the law transition year). The coefficients for the “Experience Toward Tenure (In-district)” variables show the effect of each experience category under the old law, relative to 0 years of in-district experience under the old law. The coefficients for the “Experience Towards Tenure X New Law Interactions” variables show the effect of each experience category under the new law, relative to the corresponding experience category under the old law.

is about 3 absences and the difference in Washington is about 4 absences. The overall experience point estimates generally increase monotonically with each additional year of experience, which is in line with our expectations and prior evidence on teacher absences (e.g., Clotfelter, et al., 2008).³⁵

The difference-in-difference variables we focus on here are the experience toward tenure variables interacted with the new law. Again, there is consistency across states with evidence of decreased absences in the early years (zero and 1 year of experience toward tenure in North Carolina, and zero years in Washington). When looking at these year-specific, in-district-experience and new-law interacted point estimates, we particularly focus on the estimates in the extended year of the probationary period in both states (3 years of in-district experience in North Carolina and 2 in Washington), as these estimates might be thought to show the greatest differentials between the two tenure regimes since these are teachers who would have been tenured under the old state law but have an additional probationary year under the new law in each state. In Washington, we do find statistically significant difference of about 1.1 fewer absences during the extension year. This is evidence that teacher absence behavior was affected by the law change. In North Carolina, the point estimates for these extended years are not statistically significant at conventional levels (and it is marginally significant and positive, which is in the opposite direction of the theorized effect on effort).

Columns 2 and 6 estimate the same model, adding a vector of school fixed effects to control for differences in absence that may be associated with schools, such as principals' expectations of teachers (Hansen, 2009). Though the statistical significance of some point estimates is marginally different in some of the interacted parameters in these columns, the results are qualitatively consistent with those presented in Columns 1 in the case of North Carolina. But in Washington we now see evidence that

³⁵ Though not reported here, we also estimate simpler models (see equation 1), which include a variable indicating that the teacher is in their probationary period, with interactions for the new law. We find that in both states, absences are lower for probationary teachers under the new law, relative to probationary teachers under the old tenure regime.

teachers with 1 year of experience toward tenure are less likely to be absent under the new law than the old law.³⁶

Though we do not have any strong reason to believe the types of teachers entering the workforce after the policy change in either state are significantly different from those before the change (compare the characteristics of probationary teachers in Table 3), we present the results using a cohort fixed effect in Columns 3 and 7. In this specification, which is designed to account for the potential that the new law affects the composition of teachers entering the workforce, the identifying variation is based the exposure to the new and old tenure laws across teachers in the same cohort. In effect, this is driven by teachers who begin their career under the old law and either switch districts before earning tenure or exit teaching and return to the profession under the new law. In North Carolina (Column 3), the in-district and overall experience vectors are pointing in similar directions as the earlier specifications, though the interacted variables combining the effect of experience-to-tenure and the new law are very different. The point estimate for an untenured teacher new to the district is still marginally significant and negative (consistent with Columns 1 and 2, though smaller in magnitude), yet now all of the interaction variables for one year or more towards tenure are not statistically significant. In Washington, the addition of cohort fixed effects (Column 7) does not substantially affect the results, but the coefficients on the significant interaction terms are a bit smaller.

Columns 4 and 8 present the model using teacher fixed effects. The identification for this specification is based on teachers who are under the old tenure regime at the beginning of their careers and who eventually switch to the new regime before earning tenure. As discussed in Section 3A, this transition can be due to district switches that occur during the probationary period or because teachers are from the cohort that started under the old law but could not earn tenure within the required time

³⁶ We also estimate models with school-by-year fixed effects and the results are very similar to the school fixed effects models.

frame required by the new law (1997 cohort in North Carolina and 2010 cohort in Washington). In North Carolina (Column 4), the point estimates on the vector of interacted variables are now significant and negative for the first two years, but are not statistically significant in the years leading up to tenure. After accumulating 4 or more years of in-district experience (i.e., teachers are tenured), we now estimate a statistically significant 0.5 fewer days of absence under the new law, which is a change from prior specifications. In Washington (Column 8), again we see negative point estimates for 0 years and 2 years of in-district experience.³⁷

In summary of Table 4, absence behaviors associated with the extended probationary period diverged in the two states. In Washington, these results (particularly those of the cohort and teacher fixed effects specifications) are roughly consistent with what we might expect about teacher effort if the extended time period heightened teachers' focus on exerting more effort leading up to tenure. Also, given prior evidence of effort seeing the greatest bump in the year just before tenure (Jones, 2015), our statistically significant and negative point estimate of 1 to 1.5 fewer days of absence during the extended year would support this effort hypothesis. North Carolina, on the other hand, did not conform to this hypothesis nearly as well. The number of pre-tenure absences are significantly lower under the new law, though much of the estimated differential arises in the first two years of a teacher's in-district experience where there was no actual change in tenure status between the legal regimes. And surprisingly, the extended probationary year (with 3 years of in-district experience) was either not significant or significant and positive, which is in the opposite direction of the theorized effort response.³⁸

³⁷ When we estimate simpler models that only include an indicator for Law Transition Year (3 years of district experience in North Carolina and 2 years of district experience in Washington) interacted with the new law, including controls for experience, teacher fixed effects, and year fixed effects, we find results similar to those presented in Table 4. In both the simple model and the teacher fixed effects specification in Table 4, the coefficient on the Law Transition Year interaction term is not significant in North Carolina, but is negative and statistically significant in Washington.

³⁸ Miller et al. (2008) find that a decrease in teacher absences by 1 day approximately translates to a 0.003 standard deviation increase in math student achievement. Using this conversion, we estimate that the drop in absences

B. Teacher Absence Models: Robustness and Falsification Tests

In this subsection, we describe three different tests of robustness related to: 1) ambiguities of tenure status, 2) alternative measures of teacher absences, and 3) the potential that our findings are related to factors other than the extension of time to tenure.³⁹

As we noted in the data section, ambiguity in tenure status can arise in North Carolina because districts hiring teachers who were tenured in one district but move to another may either be awarded tenure immediately or be required to serve a one-year probationary period in their new district. About 1 percent of our sample falls into this ambiguous category.⁴⁰ We do not know the extent to which districts opted to award tenure to eligible teachers who switch districts, but the above findings are based on coding the data with the assumption that these teachers were awarded tenure. To assess whether our findings are sensitive to this assumption, we estimate models that exclude observations for tenured teachers who switch districts. Our findings are not meaningfully affected by changing the assumption of districts' tenure decisions for teachers who transfer from other North Carolina districts.⁴¹

The second robustness test is for alternative absence measures. These alternate forms include: in North Carolina, sick leave only, and personal leave only; in Washington, total absences throughout the

associated with the tenure extension (1.5 days which is the largest effect we found in Table 4) would result in an increase of 0.005 standard deviations of student achievement. Though not presented here, we estimate models predicting student achievement, with the same controls used in Table 4 and in most cases find the standard errors for the tenure-related coefficients to be too large to detect statistically significant effects.

³⁹ As discussed in Section 3A, there is some concern regarding how absences are recorded in Washington in early years of the sample. Though not reported in detail here, as an additional robustness check we also estimate models similar to those presented in Table 4, but excluding the first year of data (2009) in Washington. In these models, the transition year under the new law interaction term is no longer significant in the cohort fixed effects models, but in general we find very similar results to our primary models.

⁴⁰ About 17% of the sample is tenured, and of those teachers about 5% switch districts while tenured in our sample timeframe, which translates to about 1% of the overall sample.

⁴¹ In Table 4, column 1, the coefficient on the 1 year under the new law interaction term is significant and negative, but when we drop teachers with ambiguous status this effect is no longer significant. The full set of results are not presented here but are available from the authors by request.

school year⁴² and absences falling on Mondays and Fridays; and in both states, absences occurring after standardized testing. A motivation for assessing different types of absences is the potential that some types of absences represent a more narrowly constructed measure of teacher effort because they are more a matter of teacher discretion, and/or less likely to be associated with other forms of accountability. In particular, one might hypothesize that teachers face greater scrutiny over personal leave than sick leave, and that absences on Mondays and Fridays are related to extensions of the weekend (Miller et al., 2008). There is also less student-outcome accountability pressure on teachers after state testing has occurred so they may feel less constrained about being absent.

We report estimates from models with different types of absences in **Table 5**, with analogous results from Table 4 included for comparison. For brevity, we only report estimates from models including experience toward tenure, new law interactions, and teacher fixed effects (columns 4 and 8 in Table 4).⁴³ In both states, the results for our variables of interest (the interaction between experience toward tenure and the new law) are generally consistent with the main results presented in Table 4, though the magnitudes of the point estimates differ slightly.

[Table 5 about here]

As discussed in Section 3A, the absence variable we use for the primary North Carolina models is computed by summing the number of sick leave and personal leave absences, consistent with Clotfelter et al. (2007). For columns 3 and 4, we estimate personal and sick leave absences separately and find similar results. Overall, the number of post-test absences in both states is much lower than our primary

⁴² As noted in Section 3A, our primary models in Washington exclude absences in the extension year after March 16th.

⁴³ The overall pattern of results is very similar when we use the other specifications presented in Table 4. Results from other model specifications are available from authors upon request.

absence variables, since standardized testing dates are late in the school year. Typically there are less than 40 school days after testing dates for a teacher to potentially be absent, as opposed to around 180 school days for absences throughout the whole school year. As expected, the point estimates for post-test absences are much smaller, but in the same direction as our primary results. Similarly, the point estimates for the Monday and Friday absences (available in Washington only) are in the same direction and smaller, which is expected considering Mondays and Fridays are roughly 40 percent of school days.

In column 6 we estimate the total number of absences throughout the school year, including absences after the date when teachers would expect to know whether they would be tenured under the new law (see Section 3A for more details). Interestingly, the difference in the effect under the old and new law for the group of teachers affected by the tenure extension is not significant with the “total absences” definition. This suggests that the new law reduced teacher absences before the March 16th deadline, but after teachers were given notice that they were to be retained for the following year, the number of absences increased.

Finally, a concern that arises in both states is that the findings may not be related to the time to tenure extension, but rather other unobserved factors, such as broad changes in the labor market. In North Carolina, the change in the time to tenure law was enacted as part of a larger bill intended to improve educator quality, which included mandating changes for teacher preparation programs, certification rules, evaluation standards, and altering the statewide minimum salary schedule (e.g. see Joyce, 1997 for more detail on these changes). In Washington State, the tenure law was changed as a part of legislation (SB 6696) designed to make the state’s Race to the Top application more competitive. The main focus of this legislation was on teacher evaluation; specifically it required school districts to adopt a new four-tiered teacher evaluation system. However, the new evaluation system is only required in 2013-14.

We conduct a falsification test designed to test the possibility that our primary findings are caused not by the tenure extension but by unobserved factors. While we cannot rule out the possibility that factors concurrent with the tenure extension are driving our results, but we can assess whether there is evidence that broader unobserved trends appear to be driving our extension findings with a falsification test that assumes that the tenure laws were enacted one year earlier than they actually were. For this test, we flag teachers who, in reality, were under the old law (with the shorter probationary period), but who would have been under the new law if the reform had been implemented one year earlier.⁴⁴ We interact this placebo new law indicator with the same in-district experience categories and re-estimate our primary models from Table 4, adding in the new interaction terms. Because we still include the same variables as our primary models, the coefficients on the interaction terms represent the effect of the placebo (early) extension of the probationary period, controlling for the estimated effect of the actual tenure reform. The presence of a significant effect for groups that should not be affected by the early implementation of the new law placebo would be evidence that something other than the tenure extension may be affecting the number of absences for teachers.

[Table 6 about here]

In Washington, none of the placebo new law interaction terms are significant, and we find few differences in the coefficients from our primary results, which serves as evidence that our primary results are driven by the absence behavior of teachers with extended probationary periods and not by time-specific factors that would affect all teachers. In our primary models in North Carolina, the largest reduction in absences under the new law was for teachers with 0 and 1 year of district experience, and

⁴⁴ These groups of teachers are illustrated in Table 2 as groups that are directly above the solid line separating the lighter-shaded groups under the old law from the darker-shaded groups under the new law.

the hypothetical new law interaction terms for these groups are also not significant in the falsification test, indicating that the effect on the tenure reform for early career teachers in North Carolina also appears not to be related to unobserved factors that are closely related in time with the tenure extension.⁴⁵

C. Tenure Extension and Pre-tenure Teacher Mobility

Question 2 focuses on the association between the extended probationary period and pre-tenure mobility patterns overall and for particular teachers who we may expect to be more at risk for being denied tenure. From the policymakers' perspective, a key rationale behind extending the probationary period is to enable districts to be more selective. The investigations in this section shed light on whether or not teacher mobility changed in ways that would be consistent with greater levels of teacher sorting or district selectivity, and might help explain the patterns we report in subsection 4A above. As described previously, we do not observe why teachers are moving, which may be due to the law change or for personal reasons; however, if greater selectivity leading up to tenure is playing a role, we should see differences in mobility patterns arise in relation to the policy change.

We begin by focusing on overall changes in mobility patterns in **Table 7**, which presents the results of the multinomial logit model conditioned on teachers' experience towards tenure as described in Equation (3). The two mobility outcomes are: switching districts (odd numbered columns) and leaving the state public schools (even columns).⁴⁶ The coefficients displayed in the table are average marginal

⁴⁵ In the North Carolina teacher covariates model and the school fixed effects model, the coefficient on the two years experience under the hypothetical new law interaction term is significant and negative. The fact that this coefficient is similar to the coefficient on the two years experience under the new law interaction term indicates that the effect on absences for these two groups is similar. However, the effect for teachers with two years of district experience is not significant in any of our primary models, so this group is not driving our results.

⁴⁶ Teachers are coded as leaving the state public schooling system if they do not show up in the state-level dataset in the following year, for example, if they exited the profession, taught in a public school in another state, taught in a private school, or remained in the state in a non-teaching position.

effect estimates of switching districts or leaving the state public school system, relative to remaining in the district in the following year.

[Table 7 about here]

Looking first at the overall difference-in-difference estimates in columns 1-2 and 7-8, we see that, relative to novice teachers (the reference category), more experienced teachers are far less likely to switch districts (in both states) and significantly less likely to leave North Carolina public schools. There is also evidence that untenured teachers' mobility patterns in North Carolina look markedly different after the law change, with both switching districts (2 percentage points) and leaving the state entirely (3 percentage points) showing a significant and positive association with the tenure extension. Washington teachers, on the other hand, show no such relationship associated with the policy change. For comparison, in North Carolina and Washington about 5 percent of untenured teachers in our analytic sample switch districts each year; in North Carolina, about 16 percent of untenured teachers leave the state each year and in Washington about 10 percent of untenured teachers leave the state.

When conditioning on the vector of in-district experience variables (columns 3-6 and 9-12), both states show significantly negative associations between switching districts and leaving the state and experience toward tenure under the old tenure law, with magnitudes ranging from 1 to 15 percentage points; this suggests high levels of teacher attachment in order to qualify for tenure. However, the interaction between the in-district experience variables and new tenure law only show a significantly different mobility pattern for pre-tenure teachers in North Carolina with pre-tenure teachers significantly more likely to leave the state, by about 2-3 percentage points in some specifications, during the probationary period under the new law. However, starting at the extension year, teachers are

significantly *less* likely to switch districts by 1-2 percentage points. The magnitude of the findings are also, for some in-district experience categories, sensitive to the inclusion of cohort fixed effects.

Next, we turn to the results of our logit models, presented in **Table 8**, conditioning on pre-tenure absences.⁴⁷ Table 8, Column 1 presents the estimated average marginal effect estimates predicting teacher attrition at the end of a teacher’s first year in the district in North Carolina; Column 2 presents those corresponding to second-year attrition; Column 3 is the third year; Column 4 is the fourth year (the marginally extended probationary year in NC); and Column 5 is the fifth year (the first post-tenure year under the new tenure regime). Columns 6-10 represent retention in the years 1 through 5 in Washington, representing the full probationary period plus two years following tenure under the extended probationary period.

[Table 8 about here]

We find no clear evidence of systemic changes in retention based on absences as both states extended their probationary periods. In North Carolina, both the absence variable and the indicator for being in the highest absence quintile were statistically significant and positive, showing an association with teachers’ likelihood of exiting the district. Each additional absence is associated with an increase in the probability of leaving the district by .1 to .4 percentage points, and being in the top quintile of absences increases the probability by 5 to 8 percentage points. The new law alone was associated with a modest increase in the probability of teacher attrition, at least in years 1 through 3 of district experience, with magnitudes as large as 4 percentage points. Yet, neither of the absence variables

⁴⁷ In addition to the logit models presented here, we also estimated multinomial logit models predicting teachers’ mobility choices to either stay in the district, switch districts, or leave the state public school system. The results were not qualitatively different than the logit estimates presented here, although teachers in North Carolina were more likely to exit the public school system than switch districts under the new law; we report the logit models for their relative simplicity.

interacted with the new law were found to be statistically significant in any of the years examined here. In Washington we also find no evidence of differential retention associated with the absence variables under the new law. In other words, extending the probationary period does not appear to be associated with differential selection based on these measures in either state.⁴⁸

5. Policy Implications and Conclusions

Many states' recent policy actions focused on teacher accountability have aimed at manipulating some of the job protections provided to public school teachers in the form of tenure. Extending the years of experience required to be eligible for tenure is one of the prominent ways in which these protections have been curtailed, though no prior empirical evidence has investigated the effect of such tenure changes on school teachers' effort as measured by their absence behavior, their pre-tenure mobility patterns, or whether the extra year of information that district administrators have about teachers affects the likelihood of teacher attrition.

The results presented in this paper using data from North Carolina and Washington show an association between the introduction of the extended pre-tenure probationary period and fewer teacher absences in both states. Yet, the pattern of those reductions differs across states: in Washington, we find a significant reduction of more than one day of absence in the extended probationary year, consistent with expectations; yet, in North Carolina, reductions were observed in the first two years on the job (i.e., pre-tenure years under both the new and old tenure regimes). We conducted a series of robustness checks that show the results are qualitatively similar across various specifications and absence measures, and a falsification test supports the proposition that our findings

⁴⁸ Though not reported here, as robustness checks we estimate this model using a variety of absence measures and estimating a multinomial logit predicting different types of mobility. The results are qualitatively similar to what we report in Table 8.

are indeed related to the extension of tenure in each state rather than other unobserved factors. We cannot be sure that our estimated effects are capturing effort differences between teachers only, the combined weight of the evidence suggests the relationship is plausibly causal. One avenue for future research is to investigate whether the differences in absences that are associated with the tenure extension persist or diminish over time. Additionally, it is possible that teachers are responding not to the specific policy of the tenure extension, but instead are responding to general uncertainty that accompanies any policy change. If reforms trigger feelings of instability about the teaching profession, these perceptions may be what drive changes in behavior rather than the actual reform.

We also explored teacher mobility during the extended probationary period. Our first mobility investigation showed that, all things equal, pre-tenure teachers in both states show relatively high levels of attachment to both the district and teaching in the state (controlling for overall experience separately). Yet, the extension of the probationary period was only associated with changes in pre-tenure mobility in North Carolina, showing teachers were significantly more likely to switch districts in the state in the early years of the probationary period, and then were significantly less likely to leave the state in the actual extended probationary year. We saw no evidence of changes in pre-tenure mobility among teachers in Washington. We also explored whether teachers' attrition from the district (either due to intra-state moving or exiting the state entirely) was related to the change in tenure law, based on criteria that was plausibly used to select teachers for tenure. We find general evidence of higher absences associated with greater attrition (though this relationship is stronger in North Carolina), which suggests some differential selection to tenure status. However, we did not find any change in these selection patterns associated with the implementation of the new extended probationary period in either state.

Based on this evidence, we are left to conclude that extending the probationary period in these two states appears to have had a modest transitory effect on pre-tenure teachers (on both absences

and, in North Carolina, mobility) leading up to the point of tenure, though we see no evidence that tenure was awarded more selectively under the new law. If what we see here is generalizable to other states and outcomes, this suggests that extending the tenure period may influence how teachers behave in the short term, but may have little bearing on the quality of the workforce in the long term. One might conclude that unless states and districts clearly specify new performance criteria for tenure selection (as is the case in Loeb et al., 2014), along with the extended probationary period, the extension of time to tenure alone would have a limited impact on the composition of the teacher workforce through selection. But, it is important to acknowledge that our evidence is solely related to teacher absences, highlighting the importance of studying other teacher outcomes, including on students, and how they might be affected by changes in tenure laws.

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Table 1. Tenure Requirements in North Carolina and Washington

| | North Carolina | Washington |
|--|--|---|
| Current minimum experience requirement | Four consecutive years of full-time teaching in a North Carolina public school system | Three years of full-time teaching in Washington public school system, or two years (at the superintendent's discretion) if teachers receive one of the top two evaluation ratings each year. |
| Additional requirements | <ul style="list-style-type: none"> • Must perform at least 120 days each year for time to count towards tenure • Time to tenure not portable between districts Time to tenure resets when moving to a new district • Districts hiring previously tenured teachers given discretion in granting tenure | <ul style="list-style-type: none"> • Time to tenure not portable between districts • Tenured teachers moving to new districts within state have one-year probationary period in new district |
| Policy change | <ul style="list-style-type: none"> • 1997 Excellent Schools Act extended probationary period from three to four years. • Change applied to teachers who had not attained career status prior to July 1, 1998. | <ul style="list-style-type: none"> • 2010 Senate Bill 6696 extends the probationary period from two to three years. • Superintendents are given the discretion to grant tenure after two years with good evaluations (effective 2013-14). • Change applies to teachers who have not received tenure as of 2010-11 school year. |

Table 2. Cohorts Affected by Tenure Reforms**Panel A.** North Carolina

| School Year | Years of Prior District Experience | | | | | |
|----------------|---|---|---|---|---|---|
| | 0 | 1 | 2 | 3 | 4 | 5 plus |
| 1994/95 | Untenured, Old Law for 1995 Cohort | | | | | |
| 1995/96 | Untenured, Old Law for 1996 Cohort | Untenured, Old Law for 1995 Cohort | | | | |
| 1996/97 | Untenured, Old Law for 1997 Cohort | Untenured, Old Law for 1996 Cohort | Untenured, Old Law for 1995 Cohort | | | |
| 1997/98 | Untenured, New Law for 1998 Cohort | Untenured, New Law for 1997 Cohort | Untenured, Old Law for 1996 Cohort | Tenured, Old Law for 1995 Cohort | | |
| 1998/99 | Untenured, New Law for 1999 Cohort | Untenured, New Law for 1998 Cohort | Untenured, New Law for 1997 Cohort | Tenured, Old Law for 1996 Cohort | Tenured, Old Law for 1995 Cohort | |
| 1999/2000 | Untenured, New Law for 2000 Cohort | Untenured, New Law for 1999 Cohort | Untenured, New Law for 1998 Cohort | Untenured, New Law for 1997 Cohort | Tenured, Old Law for 1996 Cohort | Tenured, Old Law for 1995 Cohort |
| 2000/2001 | Untenured, New Law for 2001 Cohort | Untenured, New Law for 2000 Cohort | Untenured, New Law for 1999 Cohort | Untenured, New Law for 1998 Cohort | Tenured, New Law for 1997 Cohort | Tenured, Old Law for 1996 Cohort |
| 2001/2002 | Untenured, New Law for 2002 Cohort | Untenured, New Law for 2001 Cohort | Untenured, New Law for 2000 Cohort | Untenured, New Law for 1999 Cohort | Tenured, New Law for 1998 Cohort | Tenured, New Law for 1997 Cohort |

Panel B. Washington

| School Year | Years of Prior District Experience | | | | | |
|-------------|--|--|---|---|---|---|
| | 0 | 1 | 2 | 3 | 4 | 5 plus |
| 2008/09 | Untenured, Old Law for 2009 Cohort | Untenured, Old Law for 2008 Cohort | Tenured, Old Law for 2007 Cohort | | | |
| 2009/10 | Untenured, Old Law for 2010 Cohort | Untenured, Old Law for 2009 Cohort | Tenured, Old Law for 2008 Cohort | Tenured, Old Law for 2007 Cohort | | |
| 2010/11 | Untenured, New Law for 2011 Cohort | Untenured, New Law for 2010 Cohort | Tenured, Old Law for 2009 Cohort | Tenured, Old Law for 2008 Cohort | Tenured, Old Law for 2007 Cohort | |
| 2011/12 | Untenured, New Law for 2012 Cohort | Untenured, New Law for 2011 Cohort | Untenured, New Law for 2010 Cohort | Tenured, Old Law for 2009 Cohort | Tenured, Old Law for 2008 Cohort | Tenured, Old Law for 2007 Cohort |
| 2012/13 | Untenured, New Law for 2013 Cohort | Untenured, New Law for 2012 Cohort | Untenured, New Law for 2011 Cohort | Tenured, New Law for 2010 Cohort | Tenured, Old Law for 2009 Cohort | Tenured, Old Law for 2008 Cohort |
| 2013/14 | Untenured, New Law for 2014 Cohort | Untenured, New Law for 2013 Cohort | Untenured, New Law for 2012 Cohort | Tenured, New Law for 2011 Cohort | Tenured, New Law for 2010 Cohort | Tenured, Old Law for 2009 Cohort |
| 2014/15 | Untenured, New Law for 2015 Cohort | Untenured, New Law for 2014 Cohort | Untenured, New Law for 2013 Cohort | Tenured, New Law for 2012 Cohort | Tenured, New Law for 2011 Cohort | Tenured, New Law for 2010 Cohort |

Note: Senate Bill 6696 was passed on March 11, 2010, so it's ambiguous whether the 2010 cohort (who started their first year of teaching in the fall of 2009) knew they were under the old or new law in their first year.

Table 3. Sample Statistics**Panel A. North Carolina (1995-2002)**

| | Old Law: Tenured after 3 Years | | New Law: Tenured after 4 Years | |
|-----------------------------------|--------------------------------|----------------|--------------------------------|----------------|
| | <u>Probationary</u> | <u>Tenured</u> | <u>Probationary</u> | <u>Tenured</u> |
| Absences | 7.07 | 10.23 | 8.03 | 10.07 |
| In District Experience/Experience | | | | |
| Toward Tenure | 0.61 | 4.28 | 0.99 | 4.27 |
| Overall Experience | 3.10 | 7.19 | 4.85 | 8.30 |
| Female | 0.75 | 0.79 | 0.73 | 0.78 |
| Black | 0.12 | 0.13 | 0.14 | 0.12 |
| Hispanic | 0.01 | 0.00 | 0.01 | 0.01 |
| White | 0.81 | 0.84 | 0.78 | 0.85 |
| Other Ethnicity | 0.01 | 0.01 | 0.01 | 0.01 |
| MA or Higher | 0.39 | 0.44 | 0.40 | 0.46 |
| Praxis 1 (math z-score) | -0.02 | -0.05 | 0.00 | 0.01 |
| Praxis 1 (reading z-score) | 0.00 | -0.20 | 0.00 | 0.09 |
| Praxis 1 (writing z-score) | 0.01 | -0.09 | 0.00 | -0.01 |
| N (teacher-years) | 32,945 | 19,194 | 108,783 | 9,045 |

Panel B. Washington (2009-2015)

| | Old Law: Tenured after 2 Years | | New Law: Tenured after 3 Years | |
|-----------------------------------|--------------------------------|----------------|--------------------------------|----------------|
| | <u>Probationary</u> | <u>Tenured</u> | <u>Probationary</u> | <u>Tenured</u> |
| Absences | 5.54 | 10.93 | 8.91 | 12.26 |
| In District Experience/Experience | | | | |
| Toward Tenure | 0.55 | 4.34 | 0.84 | 3.71 |
| Overall Experience | 3.41 | 7.21 | 3.98 | 7.44 |
| Female | 0.75 | 0.72 | 0.75 | 0.72 |
| Black | 0.01 | 0.01 | 0.01 | 0.01 |
| Hispanic | 0.06 | 0.05 | 0.05 | 0.05 |
| White | 0.88 | 0.90 | 0.89 | 0.89 |
| Other Ethnicity | 0.05 | 0.04 | 0.05 | 0.05 |
| MA or Higher | 0.41 | 0.59 | 0.47 | 0.59 |
| WEST-B (math z-score) | -0.04 | -0.09 | 0.05 | 0.05 |
| WEST-B (reading z-score) | -0.03 | 0.00 | 0.00 | 0.03 |
| WEST-B (writing z-score) | -0.05 | -0.05 | 0.03 | 0.04 |
| N (teacher-years) | 2,775 | 15,333 | 20,388 | 4,821 |

Note: The Praxis 1 and WEST-B are the basic required teacher licensure tests in North Carolina and Washington, respectively.

Table 4. Absences for North Carolina and Washington by Tenure Status

| | North Carolina (1995-2002) | | | | Washington (2009-2015) | | | |
|--|----------------------------|----------------------|---------------------|----------------------|------------------------|----------------------|----------------------|----------------------|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| Experience Toward Tenure (In-District) | | | | | | | | |
| 1 year | 1.334*** (0.138) | 1.353*** (0.138) | 0.520*** (0.152) | 1.575*** (0.170) | 0.494 (0.340) | 0.686** (0.323) | 0.28 (0.350) | 0.893** (0.409) |
| 2 years (NC) | 1.103*** (0.200) | 1.130*** (0.200) | 0.166 (0.223) | 1.756*** (0.250) | | | | |
| Law Transition Year (3 years in NC; 2 years in WA) | 0.971*** (0.261) | 0.995*** (0.261) | -0.086 (0.282) | 2.003*** (0.308) | -2.104*** (0.327) | -2.259*** (0.313) | -2.444*** (0.341) | -2.119*** (0.448) |
| 4 plus years (NC) or 3 plus years (WA) or previously tenured under old law | 1.946*** (0.239) | 1.946*** (0.240) | -0.283 (0.267) | 2.177*** (0.307) | 1.721*** (0.334) | 1.208*** (0.323) | 0.937*** (0.357) | 1.036** (0.514) |
| Experience Toward Tenure X New Law Interactions | | | | | | | | |
| 0 years under new law | -1.246*** (0.226) | -1.273*** (0.226) | -0.444* (0.244) | -0.751*** (0.254) | -1.352*** (0.342) | -1.665*** (0.330) | -0.788** (0.377) | -1.429*** (0.442) |
| 1 year under new law | -0.618*** (0.236) | -0.589** (0.236) | 0.004 (0.254) | -0.549** (0.277) | -0.45 (0.357) | -0.939*** (0.343) | 0.135 (0.397) | -0.999* (0.570) |
| 2 years under new law (NC) | -0.146 (0.169) | -0.099 (0.169) | 0.191 (0.201) | -0.308 (0.243) | | | | |
| Law Transition Year (3 years in NC; 2 years in WA) | 0.323* (0.165) | 0.414** (0.164) | 0.312 (0.201) | -0.255 (0.247) | -1.129*** (0.297) | -1.281*** (0.285) | -0.659** (0.336) | -1.541*** (0.554) |
| 4 plus years (NC) or 3 plus years (WA) or previously tenured under new law | -0.207 (0.128) | -0.093 (0.129) | 0.254 (0.164) | -0.532** (0.224) | -0.307* (0.166) | -0.202 (0.165) | 0.277 (0.228) | -0.586 (0.528) |
| Overall Experience | | | | | | | | |
| 1 year | 0.178* (0.099) | 0.222** (0.098) | 0.457*** (0.099) | 0.473*** (0.106) | 0.690*** (0.242) | 0.796*** (0.231) | 0.660*** (0.242) | 0.586** (0.297) |
| 2 years | 0.793*** (0.103) | 0.830*** (0.103) | 0.950*** (0.105) | 1.110*** (0.122) | 1.507*** (0.249) | 1.615*** (0.238) | 1.395*** (0.250) | 1.389*** (0.336) |
| 3 years | 1.252*** (0.108) | 1.276*** (0.108) | 1.291*** (0.111) | 1.561*** (0.139) | 1.880*** (0.251) | 2.120*** (0.241) | 1.708*** (0.253) | 1.764*** (0.365) |
| 4 plus years | 0.984*** (0.080) | 1.023*** (0.081) | 1.089*** (0.084) | 2.258*** (0.158) | 2.336*** (0.225) | 2.730*** (0.219) | 1.915*** (0.231) | 2.533*** (0.400) |
| Includes Teacher Characteristics (defined in note below) | Yes | Yes | Yes | No | Yes | Yes | Yes | No |
| School Fixed Effects | No | Yes | No | No | No | Yes | No | No |
| Cohort Fixed Effects | No | No | Yes | No | No | No | Yes | No |
| Teacher Fixed Effects | No | No | No | Yes | No | No | No | Yes |
| R-squared | 0.045 | 0.078 | 0.055 | 0.459 | 0.084 | 0.221 | 0.086 | 0.524 |
| N | 169,967 | 169,967 | 169,967 | 169,967 | 43,317 | 43,317 | 43,317 | 43,317 |

Notes: *p<.10, **p<.05, ***p<.01. Omitted reference category for Experience Toward Tenure and Overall Experience variables is 0 years. Effects for the Experience Toward Tenure X New Law Interactions are relative to the respective experience category under the old law. The dependent variable for all models is the number of absences in the school year. Absences in the extension year in Washington are only counted until March 15th, the date when teachers in many districts in the state are informed whether their contract will be extended. Models also include teacher-level covariates (race/ethnicity, gender, and degree level), and year fixed effects. The rows for 2 years of experience toward tenure are blank for Washington, and estimates that correspond to that category are displayed in the Law Transition Year row.

Table 5. Alternative Absence Definitions

| | North Carolina (1995-2002) | | | | Washington (2009-2015) | | | |
|--|----------------------------------|----------------------------|---------------------|------------------------|----------------------------------|------------------------|----------------------------|------------------------------------|
| | 1 Primary Absence Model | 2 Post-Test Absences | 3 Sick Leave | 4 Personal Leave | 5 Primary Absence Model | 6 Total Absences | 7 Post-Test Absences | 8 Monday/ Friday Absences |
| Experience Toward Tenure (In-District) | | | | | | | | |
| 1 year | 1.575*** (0.170) | 0.208*** (0.078) | 1.712*** (0.144) | -0.137** (0.065) | 0.494 (0.340) | 0.829** (0.422) | 0.190*** (0.054) | 0.329* (0.178) |
| 2 years (NC) | 1.756*** (0.250) | 0.268** (0.114) | 1.955*** (0.211) | -0.199** (0.096) | - | - | - | - |
| Law Transition Year (3 years in NC; 2 years in WA) | 2.003*** (0.308) | 0.297** (0.140) | 2.169*** (0.260) | -0.166 (0.119) | -2.104*** (0.327) | 0.579 (0.463) | 0.204*** (0.059) | 0.335* (0.195) |
| 4 plus years (NC) or 3 plus years (WA) or previously tenured under old law | 2.177*** (0.307) | 0.492*** (0.140) | 2.162*** (0.259) | 0.014 (0.118) | 1.721*** (0.334) | 1.031* (0.531) | 0.182*** (0.068) | 0.626*** (0.224) |
| Experience Toward Tenure X New Law Interactions | | | | | | | | |
| 0 years under new law | -1.246*** (0.226) | -0.196* (0.116) | -0.328 (0.215) | -0.423*** (0.098) | -1.352*** (0.342) | -1.523*** (0.456) | -0.178*** (0.059) | -0.442** (0.192) |
| 1 year under new law | -0.618*** (0.236) | -0.157 (0.126) | -0.311 (0.234) | -0.237** (0.107) | -0.45 (0.357) | -0.974* (0.588) | -0.259*** (0.076) | -0.136 (0.248) |
| 2 years under new law (NC) | -0.146 (0.169) | -0.05 (0.111) | -0.177 (0.205) | -0.131 (0.093) | - | - | - | - |
| Law Transition Year (3 years in NC; 2 years in WA) | 0.323* (0.165) | -0.008 (0.113) | -0.141 (0.209) | -0.115 (0.095) | -1.129*** (0.297) | -0.478 (0.572) | -0.280*** (0.074) | -0.066 (0.241) |
| 4 plus years (NC) or 3 plus years (WA) or previously tenured under new law | -0.207 (0.128) | -0.102 (0.102) | -0.322* (0.189) | -0.210** (0.086) | -0.307* (0.166) | -0.453 (0.546) | -0.255*** (0.070) | -0.132 (0.230) |
| Overall Experience (reference category is 0 years of experience) | | | | | | | | |
| 1 year | 0.178* (0.099) | 0.168*** (0.048) | 0.478*** (0.090) | -0.006 (0.041) | 0.690*** (0.242) | 0.542* (0.307) | 0.103*** (0.039) | 0.369*** (0.129) |
| 2 years | 0.793*** (0.103) | 0.329*** (0.056) | 1.021*** (0.103) | 0.088* (0.047) | 1.507*** (0.249) | 1.223*** (0.347) | 0.168*** (0.045) | 0.677*** (0.146) |
| 3 years | 1.252*** (0.108) | 0.474*** (0.063) | 1.365*** (0.118) | 0.196*** (0.054) | 1.880*** (0.251) | 1.565*** (0.377) | 0.209*** (0.048) | 0.734*** (0.159) |
| 4 plus years | 0.984*** (0.080) | 0.623*** (0.072) | 2.008*** (0.133) | 0.250*** (0.061) | 2.336*** (0.225) | 2.495*** (0.413) | 0.243*** (0.053) | 1.165*** (0.174) |
| R-squared | 0.459 | 0.361 | 0.44 | 0.477 | 0.524 | 0.521 | 0.413 | 0.535 |
| N | 169,967 | 169,967 | 169,967 | 169,967 | 43,317 | 43,317 | 43,317 | 43,317 |

Notes: *p<.10, **p<.05, ***p<.01. Omitted reference category for Experience Toward Tenure and Overall Experience variables is 0 years. Effects for the Experience Toward Tenure X New Law Interactions are relative to the respective experience category under the old law. Models also include teacher and year fixed effects, similar to columns 4 and 8 in Table 4. Columns 1 and 5 are the same as columns 4 and 8 from Table 4 and are included for comparison.

Table 6. Falsification Tests with Placebo New Law Implemented One Year Earlier

| | North Carolina (1995-2002) | | | | Washington (2009-2015) | | | |
|--|----------------------------|----------------------|---------------------|---------------------|------------------------|----------------------|----------------------|----------------------|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| Experience Toward Tenure (In-District) | | | | | | | | |
| 1 year | 1.529*** (0.204) | 1.267*** (0.219) | 0.762*** (0.229) | 1.726*** (0.237) | 0.810* (0.484) | 0.691 (0.461) | 0.658 (0.501) | 0.688 (0.508) |
| 2 years (NC) | 1.882** (0.857) | 1.663* (0.886) | 0.736 (0.856) | 1.943** (0.818) | - | - | - | - |
| Law Transition Year (3 years in NC; 2 years in WA) | -0.323 (0.467) | -0.381 (0.493) | 0.318 (0.481) | 1.699*** (0.519) | -1.595*** (0.471) | -1.969*** (0.449) | -1.953*** (0.482) | -2.137*** (0.507) |
| 4 plus years (NC) or 3 plus years (WA) or previously tenured under old law | 0.687* (0.415) | 0.621 (0.441) | 0.21 (0.415) | 2.012*** (0.468) | 1.778*** (0.466) | 1.246*** (0.451) | 1.334*** (0.475) | 0.967 (0.603) |
| Experience Toward Tenure X New Law Interactions | | | | | | | | |
| 0 years under new law | -2.528*** (0.411) | -2.288*** (0.438) | 0.108 (0.428) | -0.891* (0.500) | -1.321*** (0.474) | -1.641*** (0.458) | -0.368 (0.511) | -1.815** (0.879) |
| 1 year under new law | -2.089*** (0.417) | -1.788*** (0.442) | 0.322 (0.436) | -0.822 (0.513) | -0.732 (0.507) | -0.918* (0.491) | 0.187 (0.545) | -1.187 (0.940) |
| 2 years under new law (NC) | -2.187*** (0.758) | -1.878** (0.784) | 0.175 (0.764) | -0.6 (0.792) | - | - | - | - |
| Law Transition Year (3 years in NC; 2 years in WA) | 0.355 (0.234) | 0.513** (0.241) | 0.466* (0.276) | -0.045 (0.331) | -1.592*** (0.418) | -1.539*** (0.401) | -0.71 (0.449) | -1.922** (0.871) |
| 4 plus years (NC) or 3 plus years (WA) or previously tenured under new law | -0.204 (0.128) | -0.148 (0.133) | 0.330** (0.168) | -0.433* (0.251) | -0.314* (0.167) | -0.206 (0.165) | 0.335 (0.231) | -0.922 (0.762) |
| Experience Toward Tenure X Placebo New Law Interactions | | | | | | | | |
| 0 years under placebo new law | 0.491 (0.886) | 0.502 (0.916) | 0.801 (0.890) | 0.24 (0.866) | 0.573 (0.600) | 0.327 (0.572) | 0.94 (0.627) | -0.422 (0.914) |
| 1 year under placebo new law | 0.136 (0.892) | 0.218 (0.922) | 0.315 (0.902) | -0.098 (0.867) | -0.051 (0.621) | 0.302 (0.594) | 0.096 (0.649) | 0.295 (0.681) |
| 2 years under placebo new law | -2.386*** (0.775) | -2.157*** (0.801) | -0.051 (0.783) | -0.475 (0.797) | -0.795 (0.511) | -0.44 (0.489) | -0.2 (0.538) | -0.065 (0.554) |
| 3 years under placebo new law (NC only) | -0.017 (0.277) | 0.027 (0.285) | 0.14 (0.301) | 0.161 (0.300) | - | - | - | - |
| Overall Experience (reference category is 0 years of experience) | | | | | | | | |
| 1 year | 0.172* (0.099) | 0.424*** (0.105) | 0.447*** (0.100) | 0.472*** (0.106) | 0.688*** (0.242) | 0.795*** (0.231) | 0.659*** (0.242) | 0.589** (0.297) |
| 2 years | 0.782*** (0.104) | 1.031*** (0.110) | 0.959*** (0.106) | 1.117*** (0.123) | 1.503*** (0.249) | 1.614*** (0.238) | 1.392*** (0.250) | 1.399*** (0.336) |
| 3 years | 1.251*** (0.108) | 1.500*** (0.116) | 1.298*** (0.112) | 1.579*** (0.140) | 1.872*** (0.252) | 2.119*** (0.241) | 1.704*** (0.253) | 1.780*** (0.365) |
| 4 plus years | 0.973*** (0.080) | 1.250*** (0.089) | 1.092*** (0.084) | 2.265*** (0.159) | 2.320*** (0.226) | 2.725*** (0.219) | 1.911*** (0.231) | 2.551*** (0.401) |
| Includes Teacher Characteristics | Yes | Yes | Yes | No | Yes | Yes | Yes | No |
| School Fixed Effects | No | Yes | No | No | No | Yes | No | No |
| Cohort Fixed Effects | No | No | Yes | No | No | No | Yes | No |
| Teacher Fixed Effects | No | No | No | Yes | No | No | No | Yes |
| R-squared | 0.046 | 0.082 | 0.055 | 0.459 | 0.084 | 0.221 | 0.086 | 0.524 |
| N | 169,967 | 169,967 | 169,967 | 169,967 | 43,317 | 43,317 | 43,317 | 43,317 |

Notes: *p<.10, **p<.05, ***p<.01. Omitted reference category for Experience Toward Tenure and Overall Experience variables is 0 years. Effects for the Experience Toward Tenure X New Law (and Placebo New Law) Interactions are relative to the respective experience category under the old law. The dependent variable for all models is the number of absences in the school year. Absences in the extension year in Washington are only counted until March 15th, the date when teachers in many districts in the state are informed whether their contract will be extended. Models include the same teacher-level covariates in Table 4 and year fixed effects.

Table 7. Marginal Effect Estimates from Multinomial Logit Models Predicting Teacher Mobility

| | North Carolina (1995-2002) | | | | | | Washington (2009-2014) | | | | | |
|--|----------------------------|----------------------|--------------------------|----------------------|--------------------------|----------------------|--------------------------|--------------------|--------------------------|----------------------|---------------------------|----------------------|
| | 1 Switch Districts | 2 Leave State | 3 Switch Districts | 4 Leave State | 5 Switch Districts | 6 Leave State | 7 Switch Districts | 8 Leave State | 9 Switch Districts | 10 Leave State | 11 Switch Districts | 12 Leave State |
| Tenure Status Groups & Interaction Terms | | | | | | | | | | | | |
| Untenured | -0.010** (0.004) | 0.047*** (0.006) | | | | | 0.001 (0.007) | 0.045** (0.013) | | | | |
| New Law | 0.005 (0.003) | -0.001 (0.004) | | | | | 0.002 (0.004) | 0.001 (0.005) | | | | |
| Untenured X New Law interaction | 0.016** (0.005) | 0.034*** (0.008) | | | | | 0.005 (0.008) | -0.011 (0.014) | | | | |
| Experience Toward Tenure (In-District) | | | | | | | | | | | | |
| 1 year | | | -0.014*** (0.003) | -0.077*** (0.006) | -0.016*** (0.003) | -0.061*** (0.007) | | | -0.009 (0.011) | -0.034** (0.013) | -0.009 (0.01) | -0.031** (0.014) |
| 2 years (NC) | | | -0.013** (0.004) | -0.101*** (0.01) | -0.014** (0.004) | -0.088*** (0.011) | | | | | | |
| Law Transition Year (3 years in NC; 2 years in WA) | | | -0.008 (0.006) | -0.113*** (0.012) | -0.01 (0.006) | -0.096*** (0.014) | | | -0.017 (0.011) | -0.066*** (0.013) | -0.012 (0.01) | -0.06*** (0.014) |
| 4 plus years (NC) or 3 plus years (WA) or previously tenured under old law | | | -0.010* (0.005) | -0.146*** (0.011) | -0.018** (0.005) | -0.105*** (0.013) | | | -0.024** (0.011) | -0.063*** (0.014) | -0.022** (0.01) | -0.056*** (0.015) |
| Experience Toward Tenure X New Law Interactions | | | | | | | | | | | | |
| 1 year under new law | | | 0.004 (0.003) | 0.016** (0.007) | 0.000 (0.004) | 0.021** (0.008) | | | -0.003 (0.011) | -0.003 (0.014) | -0.012 (0.011) | -0.009 (0.015) |
| 2 years under new law (NC) | | | -0.002 (0.005) | 0.003 (0.011) | -0.01 (0.005) | 0.030** (0.012) | | | | | | |
| Law Transition Year (3 years in NC; 2 years in WA) | | | -0.012* (0.007) | -0.007 (0.013) | -0.022** (0.007) | 0.026* (0.015) | | | 0.005 (0.011) | 0.015 (0.015) | -0.014 (0.012) | 0.006 (0.016) |
| 4 plus years (NC) or 3 plus years (WA) or previously tenured under new law | | | -0.006 (0.006) | 0.014 (0.012) | -0.014** (0.007) | 0.039** (0.015) | | | 0.01 (0.012) | 0.003 (0.016) | -0.01 (0.012) | -0.006 (0.018) |
| Overall Experience (reference category is 0 years of experience) | | | | | | | | | | | | |
| 1 year | -0.007*** (0.001) | -0.046*** (0.002) | 0.002 (0.002) | -0.008** (0.003) | 0 (0.002) | 0 (0.003) | -0.008** (0.003) | 0.003 (0.008) | -0.005 (0.004) | 0.015 (0.009) | -0.006 (0.004) | 0.014 (0.009) |
| 2 years | -0.008*** (0.002) | -0.059*** (0.002) | 0.002 (0.002) | -0.008** (0.003) | -0.001 (0.002) | 0.008** (0.004) | -0.013*** (0.003) | -0.002 (0.008) | -0.007* (0.004) | 0.021** (0.010) | -0.010*** (0.003) | 0.019** (0.010) |
| 3 years | -0.014*** (0.002) | -0.059*** (0.002) | -0.005** (0.002) | -0.012*** (0.004) | -0.009*** (0.002) | 0.007* (0.004) | -0.021*** (0.003) | -0.005 (0.008) | -0.016*** (0.003) | 0.018* (0.010) | -0.019*** (0.003) | 0.017* (0.010) |
| 4 plus years | -0.019*** (0.001) | -0.070*** (0.002) | -0.011*** (0.002) | -0.029*** (0.002) | -0.014*** (0.002) | -0.015*** (0.003) | -0.041*** (0.005) | 0.011* (0.007) | -0.031*** (0.005) | 0.031*** (0.007) | -0.039*** (0.006) | 0.034*** (0.007) |
| Cohort Fixed Effects | No | No | No | No | Yes | Yes | No | No | No | No | Yes | Yes |
| N (teacher-years) | 169,967 | 169,967 | 169,967 | 169,967 | 169,967 | 169,967 | 32,500 | 32,500 | 32,500 | 32,500 | 32,500 | 32,500 |

Notes: *p<.10, **p<.05, ***p<.01. Displayed coefficients are average marginal effect estimates relative to the probability of staying in the same district in the following year. Marginal effects for Untenured and Experience Toward Tenure variables represent the effect under the old law; marginal effects for the Untenured X New Law interaction and Experience Toward Tenure X New Law Interaction variables represent the effect under the new law, relative to the old law. Marginal effects for New Law and Overall Experience categories are calculated using observed values for all other variables. All models include the same teacher covariates in Table 4 and year fixed effects. The rows for 2 years of experience toward tenure are blank for Washington, and estimates that correspond to that category are displayed in the Law Transition Year row. For comparison, about 5 percent of observations for untenured teachers switch districts in each state, 16 percent leave the state in North Carolina, and 10 percent leave the state in Washington.

Table 8. Marginal Effects on the Probability of Leaving the District (Either Switching Districts or State Public School System)

| | North Carolina | | | | | Washington | | | | |
|--|-------------------------|----------------------|----------------------|----------------------|----------------------|-------------------------|----------------------|----------------------|----------------------|----------------------|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
| | Mobility Outcome After: | | | | | Mobility Outcome After: | | | | |
| | 1st Year in District | 2nd Year in District | 3rd Year in District | 4th Year in District | 5th Year in District | 1st Year in District | 2nd Year in District | 3rd Year in District | 4th Year in District | 5th Year in District |
| New Law X Absence Interactions | | | | | | | | | | |
| New Law | 0.028*** (0.004) | 0.041*** (0.005) | 0.015*** (0.005) | 0.001 (0.005) | 0.006 (0.005) | 0.042*** (0.011) | -0.005 (0.009) | 0.009 (0.007) | 0.004 (0.008) | -0.022*** (0.008) |
| Demeaned Absences | 0.004*** (0.001) | 0.002*** (0.001) | 0.001*** (0.000) | 0.001*** (0.000) | 0.001*** (0.000) | -0.001 (0.002) | 0.000 (0.001) | 0.001 (0.001) | 0.001 (0.001) | 0.001** (0.001) |
| Top Absence Quintile | 0.047*** (0.013) | 0.077*** (0.016) | 0.076*** (0.015) | 0.058*** (0.015) | 0.052*** (0.015) | 0.078* (0.049) | 0.05 (0.035) | 0.008 (0.02) | 0.022 (0.021) | 0.007 (0.016) |
| New Law X Demeaned Absences | 0.000 (0.001) | 0.001 (0.001) | 0.000 (0.001) | 0.000 (0.000) | 0.000 (0.000) | -0.002 (0.002) | 0.001 (0.001) | -0.001 (0.001) | -0.001 (0.001) | 0.000 (0.001) |
| New Law X Top Absence Quintile | 0.017 (0.016) | 0.004 (0.018) | -0.003 (0.017) | 0.004 (0.017) | -0.002 (0.016) | 0.07 (0.057) | -0.027 (0.038) | 0.039 (0.026) | 0.045 (0.031) | 0.004 (0.025) |
| Overall Experience (reference category is 4+ years of experience) | | | | | | | | | | |
| 0 years | 0.053*** (0.004) | - | - | - | - | 0.01 (0.011) | - | - | - | - |
| 1 year | 0.045*** (0.007) | 0.055*** (0.004) | - | - | - | 0.014 (0.022) | 0.023*** (0.008) | - | - | - |
| 2 years | 0.033*** (0.008) | 0.061*** (0.007) | 0.042*** (0.004) | - | - | 0.008 (0.027) | 0.002 (0.016) | 0.029*** (0.009) | - | - |
| 3 years | 0.018** (0.009) | 0.030*** (0.008) | 0.043*** (0.007) | 0.021*** (0.004) | - | 0.001 (0.030) | 0.006 (0.019) | -0.002 (0.016) | 0.001 (0.011) | - |
| N | 68,514 | 42,308 | 39,145 | 32,844 | 28,452 | 7,720 | 7,750 | 6,375 | 5,326 | 4,480 |

Notes: *p<.10, **p<.05, ***p<.01. Displayed coefficients are average marginal effect estimates. Marginal effects for New Law and Overall Experience categories are calculated using observed values for all other variables. Marginal effects for Demeaned Absences and Top Absence Quintile represent the effect under the old law; marginal effects for the New Law and absence variable interactions represent the marginal effect under the new law relative to the effect under the old law. Models also include teacher-level covariates. Cells for experience categories with no teachers are left blank. Column 7 has more observations than Column 6 because more teachers had missing values for absences for Outcome after Year 1, which were excluded from the analysis sample.